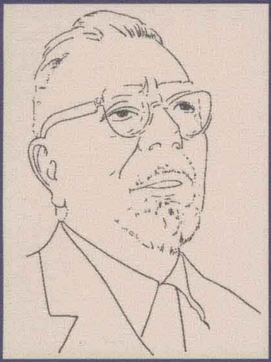

新闻学与传播学经典丛书·英文原版系列
展江　何道宽　主编

Cybernetics

or the Control and Communication in the Animal and the Machine

控制论：关于动物和机器的控制与传播科学

Norbert Wiener
[美] 诺伯特·维纳　著

中国传媒大学出版社

新闻学与传播学经典丛书·英文原版系列
展江　何道宽　主编

Cybernetics: or Control and Communication in the Animal and the Machine

控制论：关于动物和机器的控制与传播科学

Nobert Wiener（[美]诺伯特·维纳） 著

中国传媒大学出版社
2013 年

图书在版编目（CIP）数据

控制论：关于动物和机器的控制与传播科学（英文版）（美）维纳著.
—北京：中国传媒大学出版社，2013.9
（新闻学与传播学经典丛书·英文原版系列/ 展江，何道宽主编）
ISBN 978-7-5657-0770-4

Ⅰ.①控… Ⅱ.①维… Ⅲ.①控制论—英文 Ⅳ.①O231

中国版本图书馆 CIP 数据核字（2013）第 169423 号

新闻学与传播学经典丛书·英文原版系列
展 江 何道宽 主编
Cybernetics：or Control and Communication in the Animal and the Machine
控制论：关于动物和机器的控制与传播科学
Nobert Wiener（〔美〕诺伯特·维纳） 著

策划编辑 司马兰 姜颖昳
责任编辑 姜颖昳
责任印制 曹 辉
出 版 人 蔡 翔

出版发行 中国传媒大学出版社
社 址 北京市朝阳区定福庄东街 1 号 邮编：100024
电 话 86-10-65450532 或 65450528 传真：010-65779405
网 址 http：//www.cucp.com.cn
经 销 全国新华书店
印 刷 北京彩蝶印刷有限公司

开 本 880mm×1230mm 1/32
印 张 7
版 次 2013 年 9 月第 1 版 2013 年 9 月第 1 次印刷

ISBN 978-7-5657-0770-4/O·0770 **定 价** 28.00 元

版权所有 翻印必究 印装错误 负责调换

出版说明

“新闻学与传播学经典丛书·英文原版系列”，选取了在新闻学与传播学历史上具有里程碑意义的大师经典名作，如传播学“四大奠基人”哈罗德·拉斯韦尔、保罗·拉扎斯菲尔德等，及加布里埃尔·塔尔德、罗伯特·帕克、哈罗德·伊尼斯、马歇尔·麦克卢汉、库尔特·卢因、卡尔·霍夫兰等这些学界耳熟能详的名家佳作。这些是传播学与新闻学的奠基之作，也是现代新闻学与传播学发展的基础。许多名作都多次再版,影响深远，历久不衰，成为新闻学与传播学的经典。此套丛书采用英文原版出版，希望读者能读到原汁原味的著作。

随着中国高等教育的教学改革，广大师生已不满足于仅仅阅读国外图书的翻译版，他们迫切希望能读到原版图书，希望能采用国外英文原版图书进行教学，从而保证所讲授的知识体系的完整性、系统性、科学性和文字描绘的准确性。此套丛书的出版便是满足了这种需求，同时可使学生在专业技术方面尽快掌握本学科相应的外语词汇，并了解先进国家的学术发展方向。

本系列在引进英文原版图书的同时，将目录译为中文，作为对原版的一种导读，供读者阅读时参考。

从事经典著作的出版，需要出版人付出不懈的努力，好在有本丛书的主编展江教授和何道宽教授的大力扶持，我们得以在学术出版的道路上走的更远。我们自知本套丛书也许会有很多缺陷，虚心接受读者提出的批评和建议。

中国传媒大学出版社

此为试读，需要完整PDF请访问：www.ertongbook.com

目　录

第一部分　初版(1948)

第二部分　补充的几章(1961)

CONTENTS

PART I

ORIGINAL EDITION

1948

PART II

SUPPLEMENTARY CHAPTERS

1961

Introduction

This book represents the outcome, after more than a decade, of a program of work undertaken jointly with Dr. Arturo Rosenblueth, then of the Harvard Medical School and now of the Instituto Nacional de Cardiología of Mexico. In those days, Dr. Rosenblueth, who was the colleague and collaborator of the late Dr. Walter B. Cannon, conducted a monthly series of discussion meetings on scientific method. The participants were mostly young scientists at the Harvard Medical School, and we would gather for dinner about a round table in Vanderbilt Hall. The conversation was lively and unrestrained. It was not a place where it was either encouraged or made possible for anyone to stand on his dignity. After the meal, somebody—either one of our group or an invited guest—would read a paper on some scientific topic, generally one in which questions of methodology were the first consideration, or at least a leading consideration. The speaker had to run the gauntlet of an acute criticism, good-natured but unsparing. It was a perfect catharsis for half-baked ideas, insufficient self-criticism, exaggerated self-confidence, and pomposity. Those who could not stand the gaff did not return, but among the former habitués of these meetings there is more than one of us who feels that they were an important and permanent contribution to our scientific unfolding.

Not all the participants were physicians or medical scientists. One of us, a very steady member, and a great help to our discussions, was Dr. Manuel Sandoval Vallarta, a Mexican like Dr. Rosenblueth and a Professor of Physics at the Massachusetts Institute of Technology, who had been among my very first students when I came to the Institute after World War I. Dr. Vallarta used to bring some of his M.I.T. colleagues along to these discussion meetings, and it was at one of these that I first met Dr. Rosenblueth. I had been interested in the scientific method for a long time and had, in fact, been a participant in Josiah Royce's Harvard seminar on the subject in 1911–1913. Moreover, it was felt that it was essential to have someone present who could examine mathematical questions critically. I thus became an active member of the group until Dr. Rosenblueth's call to Mexico in 1944 and the general confusion of the war ended the series of meetings.

For many years Dr. Rosenblueth and I had shared the conviction that the most fruitful areas for the growth of the sciences were those which had been neglected as a no-man's land between the various established fields. Since Leibniz there has perhaps been no man who has had a full command of all the intellectual activity of his day. Since that time, science has been increasingly the task of specialists, in fields which show a tendency to grow progressively narrower. A century ago there may have been no Leibniz, but there was a Gauss, a Faraday, and a Darwin. Today there are few scholars who can call themselves mathematicians or physicists or biologists without restriction. A man may be a topologist or an acoustician or a coleopterist. He will be filled with the jargon of his field, and will know all its literature and all its ramifications, but, more frequently than not, he will regard the next subject as something belonging to his colleague three doors down the corridor, and will consider any interest in it on his own part as an unwarrantable breach of privacy.

These specialized fields are continually growing and invading new territory. The result is like what occurred when the Oregon country was being invaded simultaneously by the United States settlers, the British, the Mexicans, and the Russians—an inextricable tangle of exploration, nomenclature, and laws. There are fields of scientific work, as we shall see in the body of this book, which have been explored from the different sides of pure mathematics, statistics, electrical engineering, and neurophysiology; in which every single notion receives a separate name from each group, and in which important work has been triplicated or quadruplicated, while still other important work is delayed by the unavailability in one field of results that may have already become classical in the next field.

It is these boundary regions of science which offer the richest opportunities to the qualified investigator. They are at the same time the most refractory to the accepted techniques of mass attack and the division of labor. If the difficulty of a physiological problem is mathematical in essence, ten physiologists ignorant of mathematics will get precisely as far as one physiologist ignorant of mathematics, and no further. If a physiologist who knows no mathematics works together with a mathematician who knows no physiology, the one will be unable to state his problem in terms that the other can manipulate, and the second will be unable to put the answers in any form that the first can understand. Dr. Rosenblueth has always insisted that a proper exploration of these blank spaces on the map of science could only be made by a team of scientists, each a specialist in his own field but each possessing a thoroughly sound and trained acquaintance with the fields of his neighbors; all in the habit of working together, of knowing one another's intellectual customs, and of recognizing the significance of a colleague's new suggestion before it has taken on a full formal expression. The mathematician need not have the skill to conduct a physiological experiment, but he must have the skill to understand one, to criticize one, and to suggest one. The physiologist need not be able to prove a certain mathematical theorem, but he must be able to grasp its physiological significance and to tell the mathematician for what he should look. We had dreamed for years of an institution of independent scientists, working together in one of these backwoods of science, not as subordinates of some great executive officer, but joined by the desire, indeed by the spiritual necessity, to understand the region as a whole, and to lend one another the strength of that understanding.

We had agreed on these matters long before we had chosen the field of our joint investigations and our respective parts in them. The deciding factor in this new step was the war. I had known for a considerable time that if a national emergency should come, my function in it would be determined largely by two things: my close contact with the program of computing machines developed by Dr. Vannevar Bush, and my own joint work with Dr. Yuk Wing Lee on the design of electric networks. In fact, both proved important. In the summer of 1940, I turned a large part of my attention to the development of computing machines for the solution of partial differential equations. I had long been interested in these and had convinced myself that their chief problem, as contrasted with the ordinary differential equations so well treated by Dr. Bush on his differential analyzer, was that of the representation of functions of more than one variable. I had also become convinced that the process of scanning, as employed in television, gave the answer to that question and, in fact, that television was destined to be more useful to engineering by the introduction of such new techniques than as an independent industry.

It was clear that any scanning process must vastly increase the number of data dealt with as compared with the number of data in a problem of ordinary differential equations. To accomplish reasonable results in a reasonable time, it thus became necessary to push the speed of the elementary processes to the maximum, and to avoid interrupting the stream of these processes by steps of an essentially slower nature. It also became necessary to perform the individual processes with so high a degree of accuracy that the enormous repetition of the elementary processes should not bring about a cumulative error so great as to swamp all accuracy. Thus the following requirements were suggested:

1. That the central adding and multiplying apparatus of the computing machine should be numerical, as in an ordinary adding machine, rather than on a basis of measurement, as in the Bush differential analyzer.
2. That these mechanisms, which are essentially switching devices, should depend on electronic tubes rather than on gears or mechanical relays, in order to secure quicker action.
3. That, in accordance with the policy adopted in some existing apparatus of the Bell Telephone Laboratories, it would probably be more economical in apparatus to adopt the scale of two for addition and multiplication, rather than the scale of ten.
4. That the entire sequence of operations be laid out on the machine itself so that there should be no human intervention from the time the data were entered until the final results should be taken off, and that all logical decisions necessary for this should be built into the machine itself.
5. That the machine contain an apparatus for the storage of data which should record them quickly, hold them firmly until erasure, read them quickly, erase them quickly, and then be immediately available for the storage of new material.

These recommendations, together with tentative suggestions for the means of realizing them, were sent in to Dr. Vannevar Bush for their possible use in a war. At that stage of the preparations for war, they did not seem to have sufficiently high priority to make immediate work on them worth while. Nevertheless, they all represent ideas which have been incorporated into the modern ultra-rapid computing machine. These notions were all very much in the spirit of the thought of the time, and I do not for a moment wish to claim anything like the sole responsibility for their introduction. Nevertheless, they have proved useful, and it is my hope that my memorandum had some effect in popularizing them among engineers. At any rate, as we shall see in the body of the book, they are all ideas which are of interest in connection with the study of the nervous system.

此为试读，需要完整PDF请访问： www.ertongbook.com

This work was thus laid on the table, and, although it has not proved to be fruitless, it led to no immediate project by Dr. Rosenblueth and myself. Our actual collaboration resulted from another project, which was likewise undertaken for the purposes of the last war. At the beginning of the war, the German prestige in aviation and the defensive position of England turned the attention of many scientists to the improvement of anti-aircraft artillery. Even before the war, it had become clear that the speed of the airplane had rendered obsolete all classical methods of the direction of fire, and that it was necessary to build into the control apparatus all the computations necessary. These were rendered much more difficult by the fact that, unlike all previously encountered targets, an airplane has a velocity which is a very appreciable part of the velocity of the missile used to bring it down. Accordingly, it is exceedingly important to shoot the missile, not at the target, but in such a way that missile and target may come together in space at some time in the future. We must hence find some method of predicting the future position of the plane.

The simplest method is to extrapolate the present course of the plane along a straight line. This has much to recommend it. The more a plane doubles and curves in flight, the less is its effective velocity, the less time it has to accomplish a mission, and the longer it remains in a dangerous region. Other things being equal, a plane will fly as straight a course as possible. However, by the time the first shell has burst, other things are *not* equal, and the pilot will probably zigzag, stunt, or in some other way take evasive action.

If this action were completely at the disposal of the pilot, and the pilot were to make the sort of intelligent use of his chances that we anticipate in a good poker player, for example, he has so much opportunity to modify his expected position before the arrival of a shell that we should not reckon the chances of hitting him to be very good, except perhaps in the case of a very wasteful barrage fire. On the other hand, the pilot does *not* have a completely free chance to maneuver at his will. For one thing, he is in a plane going at an exceedingly high speed, and any too sudden deviation from his course will produce an acceleration that will render him unconscious and may disintegrate the plane. Then too, he can control the plane only by moving his control surfaces, and the new regimen of flow that is established takes some small time to develop. Even when it is fully developed, it merely changes the acceleration of the plane, and this change of acceleration must be converted, first into change of velocity and then into change of position, before it is finally effective.

Moreover, an aviator under the strain of combat conditions is scarcely in a mood to engage in any very complicated and untrammeled voluntary behavior, and is quite likely to follow out the pattern of activity in which he has been trained.

All this made an investigation of the problem of the curvilinear prediction of flight worth while, whether the results should prove favorable or unfavorable for the actual use of a control apparatus involving such curvilinear prediction. To predict the future of a curve is to carry out a certain operation on its past. The true prediction operator cannot be realized by any constructible apparatus; but there are certain operators which bear it a certain resemblance and are, in fact, realizable by apparatus which we can build. I suggested to Professor Samuel Caldwell of the Massachusetts Institute of Technology that these operators seemed worth trying, and he immediately suggested that we try them out on Dr. Bush's differential analyzer, using this as a ready-made model of the desired fire-control apparatus. We did so, with results which will be discussed in the body of this book. At any rate, I found myself engaged in a war project, in which Mr. Julian H. Bigelow and myself were partners in the investigation of the theory of prediction and of the construction of apparatus to embody these theories.

It will be seen that for the second time I had become engaged in the study of a mechanico-electrical system which was designed to usurp a specifically human function—in the first case, the execution of a complicated pattern of computation, and in the second, the forecasting of the future. In this second case, we should not avoid the discussion of the performance of certain human functions. In some fire-control apparatus, it is true, the original impulse to point comes in directly by radar, but in the more usual case, there is a human gun-pointer or a gun-trainer or both coupled into the fire-control system, and acting as an essential part of it. It is essential to know their characteristics, in order to incorporate them mathematically into the machines they control. Moreover, their target, the plane, is also humanly controlled, and it is desirable to know its performance characteristics.

Mr. Bigelow and I came to the conclusion that an extremely important factor in voluntary activity is what the control engineers term *feedback*. I shall discuss this in considerable detail in the appropriate chapters. It is enough to say here that when we desire a motion to follow a given pattern the difference between this pattern and the actually performed motion is used as a new input to cause the part regulated to move in such a way as to bring its motion closer to that given by the pattern. For example, one form of steering engine of a ship carries the reading of the wheel to an offset from the tiller, which so regulates the valves of the steering engine as to move the tiller in such a way as to turn these valves off. Thus the tiller turns so as to bring the other end of the valve-regulating offset amidships, and in that way registers the angular position of the wheel as the angular position of the tiller. Clearly, any friction or other delaying force which hampers the motion of the tiller will increase the admission of steam to the valves on one side and will decrease it on the other, in such a way as to increase the torque tending to bring the tiller to the desired position. Thus the feedback system tends to make the performance of the steering engine relatively independent of the load.

On the other hand, under certain conditions of delay, etc., a feedback that is too brusque will make the rudder overshoot, and will be followed by a feedback in the other direction, which makes the rudder overshoot still more, until the steering mechanism goes into a wild oscillation or *hunting*, and breaks down completely. In a book such as that by MacColl,[1] we find a very precise discussion of feedback, the conditions under which it is advantageous, and the conditions under which it breaks down. It is a phenomenon which we understand very thoroughly from a quantitative point of view.

Now, suppose that I pick up a lead pencil. To do this, I have to move certain muscles. However, for all of us but a few expert anatomists, we do not know what these muscles are; and even among the anatomists, there are few, if any, who can perform the act by a conscious willing in succession of the contraction of each muscle concerned. On the contrary, what we will is *to pick the pencil up*. Once we have determined on this, our motion proceeds in such a way that we may say roughly that the amount by which the pencil is not yet picked up is decreased at each stage. This part of the action is not in full consciousness.

To perform an action in such a manner, there must be a report to the nervous system, conscious or unconscious, of the amount by which we have failed to pick up the pencil at each instant. If we have our eye on the pencil, this report may be visual, at least in part, but it is more generally kinesthetic, or, to use a term now in vogue, proprioceptive. If the proprioceptive sensations are wanting and we do not replace them by a visual or other substitute, we are unable to perform the act of picking up the pencil, and find ourselves

[1] MacColl, L. A., *Fundamental Theory of Servomechanisms*, Van Nostrand, New York, 1946.

in a state of what is known as *ataxia*. An ataxia of this type is familiar in the form of syphilis of the central nervous system known as *tabes dorsalis*, where the kinesthetic sense conveyed by the spinal nerves is more or less destroyed.

However, an excessive feedback is likely to be as serious a handicap to organized activity as a defective feedback. In view of this possibility, Mr. Bigelow and myself approached Dr. Rosenblueth with a very specific question. Is there any pathological condition in which the patient, in trying to perform some voluntary act like picking up a pencil, overshoots the mark, and goes into an uncontrollable oscillation? Dr. Rosenblueth immediately answered us that there is such a well-known condition, that it is called purpose tremor, and that it is often associated with injury to the cerebellum.

We thus found a most significant confirmation of our hypothesis concerning the nature of at least some voluntary activity. It will be noted that our point of view considerably transcended that current among neurophysiologists. The central nervous system no longer appears as a self-contained organ, receiving inputs from the senses and discharging into the muscles. On the contrary, some of its most characteristic activities are explicable only as circular processes, emerging from the nervous system into the muscles, and re-entering the nervous system through the sense organs, whether they be proprioceptors or organs of the special senses. This seemed to us to mark a new step in the study of that part of neurophysiology which concerns not solely the elementary processes of nerves and synapses but the performance of the nervous system as an integrated whole.

The three of us felt that this new point of view merited a paper, which we wrote up and published.[1] Dr. Rosenblueth and I foresaw that this paper could be only a statement of program for a large body of experimental work, and we decided that if we could ever bring our plan for an interscientific institute to fruition, this topic would furnish an almost ideal center for our activity.

On the communication engineering plane, it had already become clear to Mr. Bigelow and myself that the problems of control engineering and of communication engineering were inseparable, and that they centered not around the technique of electrical engineering but around the much more fundamental notion of the message, whether this should be transmitted by electrical, mechanical, or nervous means. The message is a discrete or continuous sequence of measurable events distributed in time—precisely what is called a time series

[1] Rosenblueth, A., N. Wiener, and J. Bigelow, "Behavior, Purpose, and Teleology," *Philosophy of Science*, **10**, 18–24 (1943).

by the statisticians. The prediction of the future of a message is done by some sort of operator on its past, whether this operator is realized by a scheme of mathematical computation, or by a mechanical or electrical apparatus. In this connection, we found that the ideal prediction mechanisms which we had at first contemplated were beset by two types of error, of a roughly antagonistic nature. While the prediction apparatus which we at first designed could be made to anticipate an extremely smooth curve to any desired degree of approximation, this refinement of behavior was always attained at the cost of an increasing sensitivity. The better the apparatus was for smooth waves, the more it would be set into oscillation by small departures from smoothness, and the longer it would be before such oscillations would die out. Thus the good prediction of a smooth wave seemed to require a more delicate and sensitive apparatus than the best possible prediction of a rough curve, and the choice of the particular apparatus to be used in a specific case was dependent on the statistical nature of the phenomenon to be predicted. This interacting pair of types of error seemed to have something in common with the contrasting problems of the measure of position and of momentum to be found in the Heisenberg quantum mechanics, as described according to his Principle of Uncertainty.

Once we had clearly grasped that the solution of the problem of optimum prediction was only to be obtained by an appeal to the statistics of the time series to be predicted, it was not difficult to make what had originally seemed to be a difficulty in the theory of prediction into what was actually an efficient tool for solving the problem of prediction. Assuming the statistics of a time series, it became possible to derive an explicit expression for the mean square error of prediction by a given technique and for a given lead. Once we had this, we could translate the problem of optimum prediction to the determination of a specific operator which should reduce to a minimum a specific positive quantity dependent on this operator. Minimization problems of this type belong to a recognized branch of mathematics, the calculus of variations, and this branch has a recognized technique. With the aid of this technique, we were able to obtain an explicit best solution of the problem of predicting the future of a time series, given its statistical nature, and even further, to achieve a physical realization of this solution by a constructible apparatus.

Once we had done this, at least one problem of engineering design took on a completely new aspect. In general, engineering design has been held to be an art rather than a science. By reducing a problem of this sort to a minimization principle, we had established the subject on a far more scientific basis. It occurred to us that this was not an isolated case, but that there was a whole region of engineering work in which similar design problems could be solved by the methods of the calculus of variations.

We attacked and solved other similar problems by the same methods. Among these was the problem of the design of wave filters. We often find a message contaminated by extraneous disturbances which we call *background noise*. We then face the problem of restoring the original message, or the message under a given lead, or the message modified by a given lag, by an operator applied to the corrupted message. The optimum design of this operator and of the apparatus by which it is realized depends on the statistical nature of the message and the noise, singly and jointly. We thus have replaced in the design of wave filters processes which were formerly of an empirical and rather haphazard nature by processes with a thorough scientific justification.

In doing this, we have made of communication engineering design a statistical science, a branch of statistical mechanics. The notion of statistical mechanics has indeed been encroaching on every branch of science for more than a century. We shall see that this dominance of statistical mechanics in modern physics has a very vital significance for the interpretation of the nature of time. In the case of communication engineering, however, the significance of the statistical element is immediately apparent. The transmission of information is impossible save as a transmission of alternatives. If only one contingency is to be transmitted, then it may be sent most efficiently and with the least trouble by sending no message at all. The telegraph and the telephone can perform their function only if the messages they transmit are continually varied in a manner not completely determined by their past, and can be designed effectively only if the variation of these messages conforms to some sort of statistical regularity.

To cover this aspect of communication engineering, we had to develop a statistical theory of the *amount of information*, in which the unit amount of information was that transmitted as a single decision between equally probable alternatives. This idea occurred at about the same time to several writers, among them the statistician R. A. Fisher, Dr. Shannon of the Bell Telephone Laboratories, and the author. Fisher's motive in studying this subject is to be found in classical statistical theory; that of Shannon in the problem of coding information; and that of the author in the problem of noise and

此为试读，需要完整PDF请访问：www.ertongbook.com